METHOD & ANALYSIS IN ORGANIZATIONAL RESEARCH

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Preface

The study of organizations today includes a broad range of topics and levels of analysis, from a concern with individual behavior and attitudes of employees to a more "macro"-analytic focus on how organizations interact with their environments. Such diversity of topical interests is essential in our movement toward more informed understanding of organizational functioning. One major element that most organizational scientists share cuts across this wide array of interests: an appreciation of the need for a solid foundation in the essentials of research methods and scientific inquiry. It is this element, this striving for validity in the execution of organizational research, that serves as the focus of this book.

Our interest in putting together this volume was prompted by our concerns with the usual, dysfunctional segregation of research design and data-analytic strategies found across most methodology courses and books, and by the relatively few volumes in existence which deal explicitly with organizational research. We believe the present volume has notable features relatively unaddressed, or absent, in other related books. First, the book takes a fairly comprehensive approach in coverage of both research design and data analysis techniques. Many of the articles provide exemplary studies illustrating the application of a certain approach or technique which makes the concept "come to life," as opposed to just an abstract description. It is too often the case in statistics courses that we learn the important principles and become good technicians but lose sight of the larger picture of what the technique is actually doing, how it is applied, and under what conditions one procedure is preferred over another.

A second feature of the book, congruent with the attempt at reasonably broad coverage, is the inclusion of five especially prepared papers. These invited papers cover topics we felt could be better presented with new statements than with existing publications. Traditional topics including psychometrics and laboratory experimentation are given fresh, succinct treatments, and newer and/or controversial issues including multiple regression as a general analytic system, moderator analysis, and exploratory data analysis are discussed in chapters that may be as useful to experienced researchers as to the graduate student just beginning a research career.

Another timely feature of the book is the sizable section devoted to qualitative research methods. While the predominant thinking for many years has been that collecting quantifiable data and subjecting it to rigorous statistical tests is the way organizational science should be conducted, we are beginning to see a re-emergence of interest in "qualitative" research methods such as interviewing, unobtrusive measures, and participant observation. To be sure, tradeoffs exist in the exclusive use of either qualitative or quantitative approaches, but the data richness and depth of understanding potentially achieved through the appropriate use of qualitative methods should not be overlooked or understated. As the readings in this section suggest, perhaps we should more often employ multiple approaches.

The collection of readings does not, of course, exhaust all of the possible research issues and techniques. Although many useful articles presented themselves, length limitations prevented their more wholesale inclusion. We do feel, however, that the domain of organizational research methods and analysis has been representatively sampled.

The book is divided into six chapters. Each chapter consists of a number of readings, some introductory paragraphs, and a list of suggested further readings. Readings were chosen for their contributions as lucid descriptions of research processes or techniques, or as exemplary illustrations of actual applications. Suggested readings then broaden the range of exploration into other important or illustrative statements about the design and conduct of organizational research.

Chapter 1 presents several readings about some of the early considerations in the launching of a research project. The section begins by identifying some of the features which characterize significant research ideas, thus aiding the researcher in deciding whether or not to pursue an idea. The second reading then provides an overview of how the idea might be pursued, laying out the strengths and weaknesses of various general approaches to research. The remaining readings in the first chapter explore more specific topics of hypothesis creation and testing.

Chapter 2 includes papers addressing issues and techniques of data analysis. Readings about psychometric properties of data, exploratory data analysis, and techniques of multivariate analysis provide the reader with a broad base of information helpful for understanding research data.

Chapters 3 and 4 discuss the design of research, including numerous examples of well-conceived ideas and applications of statistical techniques. Chapter 3 concerns research in laboratory settings, including simulations. Chapter 4 moves into field settings, and includes experimental, quasi-experimental, and nonexperimental designs.

Chapter 5 also concerns field research, but the emphasis is on "qualitative" rather than "quantitative" approaches. This chapter significantly broadens the traditional research perspective which is sometimes rather constraining, and should provide some inspiration toward creativity in approach and the "mixing" of methods. Finally, Chapter 6 contains some concluding (though by no means parenthetical) thoughts about ethics, imperfect rigor, and a challenge for future learning.

We wish to express our gratitude to a number of people who provided various forms of contributions at different stages in the development of this book. We are grateful to Rick Mowday and Lyman Porter for their encouragement and support in the very early phases, when we were struggling with the decision of whether or not to "press onward."

A readings book on such a narrow topic, targeted primarily toward small graduate seminars and faculty libraries, would be accurately construed as having a fairly small market. Publishers are often a bit cautious in their receptivity to such volumes. Despite such valid concerns, Reston Publishing Company developed a keen interest early on.

The assistance of our editors, Ted Buchholtz and Greg Woods, is acknowledged and appreciated.

Our sincerest thanks go to our colleagues who devoted their time, energies, and considerable skills to the preparation of the invited papers. Each paper appropriately meets a need and strengthens the book significantly. Particularly considering the deadline pressures and other demands on their time, we were doubly pleased by their interest in and contributions to the final product.

Finally, we acknowledge the spirit of research that surrounds us in our Department of Management and College of Business Administration at Texas A&M. We continue to learn about and enjoy research, both directly and vicariously, through the faculty and administrators with whom we interact on a regular basis. Their varied pursuits and skills as organizational scientists provide a climate from which we have benefited in many ways, including the development of this collection.

Thomas S. Bateman

Gerald R. Ferris

College Station, Texas
July, 1983

Contents

ONE

Fundamental Considerations in Organizational Research

The first selection of readings presents some discussion of a number of basic considerations in the conduct of research. Topics range from the early identification of research ideas which may prove most (and least) fruitful, through the generation and testing of research hypotheses. These and other considerations in this chapter constitute some of the crucial, early decisions in the planning and execution of research projects.

To be sure, proper research planning entails explicit consideration of many choice points along the route from idea conception to the reporting of results. Ideally, these choices are made prior to the actual implementation of the study. In practice, though, contingencies may arise which necessitate (sometimes ultimately for the better) departure from plans. The ability to adapt to unexpected constraints (and opportunities) is an important asset to the investigator.

Nonetheless, certain preliminary decisions often set the parameters within which the research process must unfold. Many of these key issues are represented in Chapter 1. Unfortunately, they are so basic (as in foundational, not as in simple) that they are often not as salient to the researcher as, for example, decisions concerning data-analytic strategies. As such, these considerations are sometimes given relatively little attention, when in fact their explicit incorporation into the research plan can only strengthen the final product.

The opening piece by Daft provides information useful in making that crucial decision about an idea: whether or not to pursue it. Daft suggests that too much organizational research is dull, uninteresting, and insignificant, and he seeks to identify and thereby promote the process by which scholars become engaged in truly significant pursuits. Significant research, he concludes, is characterized by an important duality of mechanistic and organic processes. Daft's piece is not only about research, it *is* research tackling an open question and ending up with useful guidelines toward maximizing the probabilities of making important contributions to knowledge. The message is both practical and inspirational.

Scientific thinking causes one to question statements proposed as truth, not accept things on blind faith, consider alternative explanations of events and relationships, and appreciate and use valid means of acquiring new knowledge. Scott's paper provides a general, solid overview of research strategies, from naturalistic observation to carefully controlled experimentation. He nicely describes the role of language in science, and then introduces fundamental concepts including operational definitions, independent and dependent variables, confounding variables and control, factorial designs, within-and between-subjects designs, main and interactive effects, and the examination and interpretation of data. Descriptions and examples of such concepts, and delineation of the major strengths and weaknesses of various methods, provide a good foundation for more detailed analyses of specific topics later in the book.

After this general treatment of research methods, the remaining articles become much more topic-specific. Two readings address aspects of hypothesis-generation. Lundberg describes, among other things, various sources of hypotheses and the basic forms they can assume. Platt then describes a systematic approach to generating alternative hypotheses and sequential hypotheses in a continuous research program. Although Platt's field(s) are different from ours, and some might argue that the "crucial experiment" is impossible for us to attain, we can benefit from much of Platt's thinking. His article provides insight into such conceptual arenas as the power of alternative hypotheses, scientific advancement through disproof, deduction and induction, symptoms of false science, method and problem orientations, the value of the notebook method, the importance of *thinking*, and the potential utility of modeling other sciences. There is also a source of comfort to go with some of Platt's inspirational messages in his statement that success is often due to a systematic approach to research "as much as to rare and unattainable intellectual power." Taken together, these two articles about hypothesis creation nicely address a topic that receives relatively little attention in print. The reader might also consider the contributions which Lundberg's and Platt's recommendations might make to the generation of significant research as defined by Daft.

Once hypotheses are generated they must, if deemed significant, be tested. Lykken addresses some important issues in hypothesis-testing. Statistical significance, he rightly suggests, is not as important as effect size. He goes on to suggest ways in which statistical tools are misused or misinterpreted. He also discusses the importance of replication/crossvalidations. On balance, Lykken succeeds in "raising consciousness" toward increased skepticism in reading and evaluating research reports.

Suggestions for Further Reading

Bakkan, D. "The Test of Significance in Psychological Research." *Psychological Bulletin,* 1966, *66,* 423–437.

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Dubin, R. *Theory Building.* New York: The Free Press, 1978.

Dunnette, M. D. "Fads, Fashions, and Folderol in Psychology." *American Psychologist,* 1966, *21,* 343–352.

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Kaplan, A. *The Conduct of Inquiry: Methodology for Behavioral Science.* New York: Thomas Y. Crowell, 1964.

Mackenzie, K. D., & House, R. "Paradigm Development in the Social Sciences: A Proposed Research Strategy." *Academy of Management Review,* 1978, 3, 7–23.

Mowday, R. T., & Steers, R. M. *Research in Organizations: Issues and Controversies.* Santa Monica, CA: Goodyear, 1979.

Roberts, K. H., Hulin, C. L., & Rousseau, D. M. *Developing an Interdisciplinary Science of Organizations.* San Francisco: Jossey-Bass, 1978.

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Antecedents of Significant and Not-So-Significant Organizational Research[1]

Richard L. Daft

What is the process by which scholars become engaged in significant organizational research? How can a researcher identify innovative research ideas that will result in substantial increments to knowledge? These are difficult questions. They deal with the very essence of organizational research. Indeed, definitive answers to these questions may not be possible. Significant research may be the result of chance, or luck, or experience and judgment. Significant research projects may originate in the intuitive, nonlinear processes deep in the minds of investigators. We simply don't know. The purpose of this paper is to at least raise these questions, and to begin the search for answers about the origins of significant research.

Understanding how we become engaged in significant research also means understanding how we become engaged in not-so-significant research. And a substantial amount of not-so-significant research is available for examination. A common criticism of organizational behavior research is that it is dull. Research outcomes too often are neither interesting nor significant. The problems chosen for investigation often are already well researched and trivial.

The notion of research significance affects us all. We have to make choices about which research projects to undertake, and our decisions often weigh the eventual outcomes and publishability of the research. And after the research is undertaken, there are endless evaluations: by colleagues who read drafts and provide criticisms; by journal referees; perhaps by journal editors; by department heads; by promotion and tenure committees; by readers of the journal; and by other scholars doing research in the same field. Various devices are employed to assess the research, which include journal quality, number of citations, and creativity. We are continuously involved in the evaluation of research significance. And these evaluations typically are very imperfect. They do not occur until after the research act is completed, and they provoke a great deal of uncertainty and dissatisfaction because research quality is poorly understood.

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[1] This paper is adapted from chapter four in John P. Campbell, Richard L. Daft, and Charles L. Hulin, *What to Study: Generating and Developing Research Questions* (Beverly Hills, CA: Sage Publications, Inc, 1982). Readers are encouraged to consult the book and the series in which it was published: *Studying Organizations: Innovations in Methodology* (Beverly Hills, CA: Sage 1983).

Special thanks to John Campbell, Chuck Hulin and Vic Vroom who collaborated on the research design and data collection for this project. This research was supported by the Office of Naval Research and the National Institute of Education as part of the "Innovations in Methodology" project, J. Richard Hackman, Principal Investigator.

The research described in this paper has two goals. The first is to directly compare significant and not-so-significant research projects along a number of specific dimensions. This comparison may help identify characteristics of significant organizational research. Improved clarification and definition of significant research may result. The second goal of this paper is to develop criteria for predicting significant research *in advance.* In other words, what should an investigator look for when choosing a research project in order to enhance the probability that it will make a significant contribution to knowledge. To meet this end, research projects were traced back to their origins. The antecedents of significant organizational research is something about which we know almost nothing, and this is the activity we must begin to understand. An "early warning" system of sorts may be helpful if we are to improve the significance of organizational research undertaken and eventually submitted for publication.

THEORY

Good research papers might be characterized by such things as good writing, novel ideas, a clever methodology, or the integration of different concepts into a single study. These characteristics make sense, and two recent papers offer particularly useful perspectives about characteristics of research outcomes. These two papers are the theoretical starting point for the study described here.

In 1971, Murray Davis proposed an intriguing idea. He argued that sociological theories which have significant impact are those that are "interesting." Davis claimed that impact and significance have little to do with truth or empirical proof. Indeed, verifiable theories are soon forgotten. A theorist or a piece of work is considered great simply because the work is interesting.

In order to be interesting, Davis said the theory has to deny certain assumptions of the audience. If all assumptions are denied, then the theory will be seen as unbelievable or irrelevant. If no assumptions are denied, the theory will be seen as obvious, as replowing old ground. The theory must be in the middle. The theory must differ moderately from readers' assumptions in order to surprise and intrigue them. From an analysis of the sociological literature, Davis developed 12 propositions that described when theories would deny some assumptions of the audience and hence be perceived as interesting. Example theories are when the assumed independent variable in a causal relationship is shown to be a dependent variable, when assumed covariation in a positive direction between phenomena is shown to be in a negative direction, when a phenomena which appears to be ineffective is shown to be effective, or when unrelated phenomena actually are found to have a single underlying theme, and so on. Theories which have one of these characteristics will tend to be noticed and will have impact. The contribution of Davis, then, is that he went beyond the notion of "newness" in research, and described 12 explicit characteristics of research ideas that might be considered in advance. If a study is simply designed to reaffirm the assumption set of the audience, then it is not likely to be very significant.

The other study is from psychology. Stephen Gottfredson (1978) examined the peer evaluation system by collecting opinions about articles from psychology journals. This was an empirical, cross-sectional study that included 83 statements describing attributes of journal articles, evaluations by 299 qualified scholars, and articles from 9 psychology journals. His research provided a comprehensive list of article characteristics, and the results indicated that judges are highly reliable when estimating the quality of articles. The 83 items were summarized in 9 scales. Key characteristics were called originality, stylistic/compositional qualities, ho-hum research, whether the paper provided direction for new research, and the type of substantive contribution.

The work of both Davis and Gottfredson dealt with evaluation after the fact. Their research explained the success of already published papers. But one comes from sociology and one from psychology, one pertains to theories and the other to empirical papers. Within these two studies is a beginning from which to start exploring the antecedents as well as the characteristics of significant research. From this starting point we may be able to identify criteria that predict early in a project whether research is likely to be perceived as original, whether it denies the assumptions of the audience, or whether it will be simply more of the same ho-hum research that is now being published.

METHOD

The purpose of the methodology was to develop a direct comparison between significant and not-so-significant research at two stages—(1) the initial circumstances under which the research was undertaken, and (2) the published research outcomes. This meant measuring the beginning and ending of research projects. Learning about the research beginnings required personal interviews. Controlling for differences in research experience and creativity was also important, so a "within person" research strategy was adopted. Each investigator would be interviewed twice, once for a project considered significant, and once for a project considered not-so-significant.

Criterion of Significance

The first problem concerns the definition of significant and not-so-significant research. The researcher was asked to select a research project in each category (significant and not-so-significant), using acceptance by colleagues in the field as the criterion. Investigators are aware of feedback in the form of journal reviews, acceptance by colleagues, citations, and whether the work has been recognized as making a significant contribution to the field. Likewise, the researcher would know that a not-so-significant project had not been accepted in a positive manner by colleagues, had not received recognition, and perhaps was never published even though submitted to journals. Thus respondents chose the research projects about which they were interviewed, and they used acceptance by other scholars in the discipline as the criterion of significance.

Closed-ended Questions

The next step was to develop a list of specific dimensions along which to compare the research projects. This involved two steps. The first step was a literature search for dimensions along which successful projects might be discriminated from non-successful projects. This search included both the literature on research (Davis, 1971; Gottfredson, 1978), and the literature on organizational innovation which had used the method of comparing successful and unsuccessful projects (Zand and Sorensen, 1975; Science Policy Research Unit, 1972). The second step was a survey of colleagues, who were asked to provide their description of what characterized exceptionally good research papers and exceptionally poor research papers. From these sources a pool of 38 questionnaire items was developed which seemed to capture most characteristics by which significant and not-so-significant research projects would differ from one another.

Open-ended Questions

The open-ended questions were designed to trace each project back to its beginning. The formal interview was only semistructured. Since the antecedents of research is a poorly understood topic, open-ended questions provided an opportunity to explore and discover important differences between the early stages of significant and not-so-significant research outcomes. General questions were asked, and then the interviewer probed until he understood the history of the project. Example questions included: How did the project originate? Where did the idea come from? How was it developed? What attracted you to the project? What contextual factors facilitated or inhibited development of the research? Responses to these questions were written down by the interviewer for later content analysis.

Sample and Procedure

The respondents were a convenience sample of 29 scholars. The sample included all participants in the Innovations in Methodology Conference working sessions at the Center for Creative Leadership in Greensboro, N.C., August 1980. Additional researchers were interviewed at the University of Minnesota and Texas A&M University. The criterion for selection was that the scholar had done organizational research, was recognized as a capable scholar, and had completed research projects that could be considered significant and not-so-significant. Respondents were interviewed face-to-face for the open-ended questions, and then they completed the 38 closed-ended items by themselves.

Caveat

After examination of the set of 29 written pairs of interviews, it became clear that the interview team had not maintained rigorous consistency. In a few cases respondents selected projects they especially liked or disliked as the criterion of significance, with only secondary regard for acceptance or rejection by the larger academic community. Different kinds of studies also were included. Most were oriented toward theory development, but a few were oriented toward methodology. These problems reflected the exploratory nature of the research. On balance, the interviews yielded robust information. Most interviews did pertain to research that was theoretical rather than methodological, and the definition of significance in most cases was influenced by the response of the academic community rather than by the respondent's own taste. And the field interview team did conduct the interviews in a sufficiently coordinated way so that initial comparisons and insights were possible.

RESULTS

Open-ended Responses

SIGNIFICANT RESEARCH PROJECTS. Excerpts from the open-ended interviews about the origin of significant research projects are in Table 1. The paraphrases provide examples of the imagery associated with significant research projects as described by the respondents. *The reader is urged to read the items in Table 1 before reading the author's interpretation that follows.* Content analysis of the descriptions suggest several antecedents to successful research.

1. *Activity.* Significant research was an outcome of investigator activity and exposure. Frequent interactions, being in the right place at the right time, chance, and contact with management and with colleagues are related to the beginning of good research ideas. Investigator solitude and isolation probably would be less likely to result in significant research outcomes.

2. *Convergence.* There is a sense that several activities or interests converge at the same time. This convergence might include an idea with a method, or the interest of a colleague or student with exposure to an organizational problem or a new technique. Convergence seems related to the notion of activity because it is through activity and exposure that the investigator is able to be at the convergence point of several events.

3. *Intuition.* The importance of the research and the interest in it seem to be

TABLE 1

Origins of Significant Research Projects

I threw out an idea in a doctoral seminar to which a student responded. Sense of great excitement, engagement in task, reading, thinking, interacting. Continuous interaction to test ideas against one another—couldn't let go. Original idea came from interaction with executives and learning the problems they faced.

Study evolved from 2-3 streams; libertarian view, visit with ______ who had similar ideas or conclusions, endless informal discussion, previous studies I had done, observation of people.

Wanted to develop theoretical rationale and interpretation for the seemingly confusing and contradictory empirical results concerning ______. Wanted to clarify and make sense of it, and colleagues agreed with importance.

Theoretically eloquent. Idea originated in a seminar where diverse backgrounds led to stimulating clashes. Connections plus enthusiasm.

Novel combination—new theoretical idea with interesting way to test it. Also did pilot study and boom, discovered a new factor that limited previous research.

Worked in ______, and personal experience contrasted with academic theory. Findings were politically relevant for understanding motivations of poor people.

We were playing bridge with a couple from marketing. He mentioned a problem, and I said that sounds like ______ theory. We got very excited. Solved an applied problem.

I was perplexed by some results, and at the same time I read a paper by someone else who had observed the same thing and was perplexed. Tested ideas to show that conventional wisdom was erroneous and provided much simpler strategy for prediction models.

I was the entrepreneur. I perceived the need and felt it was timely. I listened to clients and sensed their careers.

Real world problems that could have policy implications. Also personal values—concern over Viet Nam war. Came from real life experience and reflection and not from literature.

The intent was to discover correct dimensions of the concept and clarify it for the literature. The concept was poorly conceptualized and was relevant to management.

Student had done an excellent paper. Decided spontaneously to collaborate with student, and was not an outgrowth of long term interests.

The idea occurred as a result of studying literature relevant to this problem. Also playful, exploratory intellectual climate—lots of "what if" conversation.

Chance. It was a matter of being in a place that did this kind of work and having the right previous experience. Had both applied and theoretical implications.

Convergence of several things. Previous book, interest in this industry, interest in ______, wife's career, and ability to use new technique.

Grew from literature review, work over previous years, interaction with Ph.D student, mathematical model developed previously, and the situation in which all this could be chunked.

From frustration over issues of motivation; from wondering how to motivate employees; from previous study of supervisors; from current literature.

A colleague walked in one day and tried to explain a new concept from operations research. Suddenly I realized significance for organizations. Multiple implication blossomed right in front of us. We talked for a year, were very excited, and finally wrote everything down.

It was a plight. I didn't believe in ______ and wanted to show it. I could use a high-powered methodology I had been taught. I meshed all my interests—small groups, personality, creativity, applied.

Dramatic topic and of interest to my associates. Methodology was of long standing interest to me. Current events influenced thinking, as did one or two key books.

guided by intuition and feeling rather than by logical analysis. Investigators often expressed a feeling of excitement or commitment, a perceived certainty, as if they "knew" at a deeper level they were doing the right thing. A great deal of intrinsic interest is also present. Logical decisison processes or planning were typically not used to select research that turned out to be significant.

4. *Theory.* A concern with theory also seems to be important. A primary goal often was to understand or explain something about organizational behavior. The investigator was curious, was concerned with a